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ON

THE RISE OF LAND IN SCANDINAVIA

BY

S. A. SEXE.

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That large tracts of Scandinavia — Sweden and Norway — have since the socalled Glacial Period risen so as to make the sea apparently sink, in some places as much as 600 feet, is an opinion in which geologists at present concur. The facts upon which this opinion is founded are, briefly stated, the following: ancient coast-lines in various heights above the present level of the sea, cut partly into solid rock, partly into loose ground, — terraces or detritus banks with even and horizontal or nearly horizontal surfaces, succeeding one another like stairs, from the rivers' mouths into the sea up along their nether course, — marine shells occurring on various levels above the modern coast-line, — and marks, cut some time ago into solid rocks along the northern part of the coast of Sweden on a level with the Baltic, now lying in various, not very great, heights above the latter.

Geologists, however, do not agree, either about the way in which the upheaval took place, or about the time it required.

Sir Charles Lyell imagines the rising to have been very slow and, moreover, interrupted by long, secular, pauses 1); consequently he finds, that an immense space of time must lie between the moment, when the marine shells now fossil began to be raised above the level of the sea, and the moment, when they attained a height of 600 feet above it. Starting from the supposition, based upon several changes of level measured on the coast of Sweden, that the mean rise

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1) Principles of Geology 10th ed. vol. II p. 196.

of Scandinavia would amount to 2½ feet in a century, he finds that the period lying between the aforesaid moments may be calculated at some 24,000 years at least.1)

Professor Keilhau in his paper on the rise of land2) expresses himself on the elevation in Norway as follows: „The changes“ (of level) „have taken place in the course of a period of probably very long duration, and they have alternated with epochs of complete repose. The latter supposition is borne out especially by the evidence of the ancient coast-lines on various levels, which could not have been formed, in case there had been a constant displacement of the points of contact between sea and land. That, even supposing the change of level to have gone on continuously, a very considerable length of time must have been consumed, for the sum of the displacements to amount to 5 or 6 hundreds of feet, may be inferred from our knowledge of the present formation of beds of clay and muscles: they increase but slowly, and there is no reason whatever to suppose that their deposition or accumulation should have gone on more quickly in former times.“

Inferring from the ancient coast-lines and terraces which he met with in Finmark and referring to Keilhau's treatise on the rise of land, the French inquirer M. Bravais thinks that the upheaval took place through sudden starts, alternating with periods of complete repose, and that it differed in the different places, so that points, once contemporaneously lying on a level with the sea, after the upheaval lie in a wavy line, which is burst in some places.

Professor Kjerulf4) is of opinion that the elevation was effected through quick upward thrusts with intermediate pauses of no very great duration; accordingly he thinks that, geologically speaking, no very long interval can lie between the litoral deposition of the raised shellbanks and the point of time when they attained a height of 600 feet above it. He thinks that his observations, together with those of Keilhau and Bravais relating to the phenomenon of the terraces, contain a protest against the infinite chronology, which had been built upon the uniform rising. During a uniform rising, according to him, no terraces could be

1) Antiquity of Man 2d ed. p. 58.

2) Nyt Magazin for Naturvidenskaberne 1ste Bd. 2det og 3die Hefte p. 253.

3) Comptes rendus X and XV.

4) On the Terraces in Norway and their Importance for the Chronology up to the Glacial Period. („Om Terrasserne i Norge og deres Betydning for Tidsregningen tilbage til Istiden.“)

formed; it would occasion an uninterrupted slope, from the height of some 600 feet down to the sea. Every terrace bears witness to an intermission, and as to the duration of the intermissions he observes, on the occasion of the assertion of Keilhau quoted above: „It is a well known fact, that Keilhau was an opponent to the glacier theory. But it is now proved that the country was once covered with inland-ice. This ice has once been melted down, and it has not been proved as yet, that the melting consumed immense lengths of time. On the contrary, it cannot be denied, that a single flood during the melting must have been able to do work for quiet times of long duration. There is then really reason to suppose the accumulations to have gone on more quickly in earlier times."

The author of these lines has often had the opportunity of seeing and surveying the terraces along our rivercourses, especially in the years 1841—1846, when I made official journeys for mineralogical purposes in the country north of the Dovre. The question of the manner of their formation often obtruded itself, but I could never come to an agreement with myself about the answer, and after examining whatever respecting the rise of the land and the formation of terraces I through a long series of years have been able to get hold of, I still stand doubtful, not about the fact of the rising but about the way in which the land rose, and the space of time required.

Regarding the ancient coast-lines cut into solid rock, I shall observe, that, in as far as they are looked upon as cut by icebergs, a glacier of great thickness — on a Greenland scale, some 1000 feet and upwards — running out through a deep fjord or through a deep sound, must needs go deep down into the water, at the same time as its surface may lie high above the water. It may thus furrow or grind the rocks deep under the sea-level, in the sea-level, and high above it. The same may be said of an iceberg drifting along and touching the rocky coast of a land. Thus, if horizontal grindings-in are found at different heights on the same rock or on different rocks, it cannot be known whether they originated cotemporarily or at different times, unless the difference of level be so great as to make it inconceivable that they could have been wrought by one and the same glacier. If horizontal furrows are met with on the same level in different places, it cannot, without further evidence, be inferred that they have originated cotemporarily. For one furrow may have been ground into the rock deep under water at one time, and the other may, after some rising of the land, have originated later, either less deep under water, or on a level with its surface, or above it. Consequently, it is not to be relied upon that furrows, cut into the rock and parallel to the present coast-line, are really ancient coast-lines ɔ: formed in what was the level of the sea at the time of the formation.

If the ancient coast-lines are to be looked upon as cut into the rocks by the erosion of the sea or by common fjord-ice, it lies near to look for their analogon on the rocks in the present sea-level which indeed, as regards Norway, is assumed to have remained unchanged for a long space of time. But I cannot say that I have seen any such incision there, which I could imagine as a future ancient coast-line, in case the land should rise in future, although I have travelled not a little, both on our fjords and along the coast facing the open sea. On the contrary, I must imagine that, if the land rose, the present rocky shore, clean-washed and in several places somewhat smoothed by the sand-mingled surf as it is, would, in the course of years, under the influence of air, water, and changes of temperature, lose every trace of the contact with the sea, thus telling nothing about its former situation to the same. However, it is conceivable, that the sea can by erosion make some kind of incision into the rocky shore, either if its level remain stationary for ages, or even if its surface should, during a slow rising of the land, come into a shorter contact with a zone of rocks, which is more easily eroded than what lies above and below. Such a zone may also get a cut by weathering after emerging from the sea. An incision in the rock, running above the sea-level and parallel to it, cannot, therefore, be looked upon as an evidence of an intermission of the rise of land, till it has been made out, that the incision did not take place in a zone, which gave more way to the erosion and the weathering than what lies below and above. To satisfy one's self on this point is, however, very often, now difficult, now impossible, our rocky coast being in many and long tracts so steep as to make it, now difficult, now impossible, to get to the place where the incision must lie. In this connexion it may be observed, that, from a distant point of view, one risks to take incisions in the rock that are casual — if such an expression be allowed — for furrows of erosion.

The ancient coast-lines, cut into loose ground, may, I suppose, be ranged with the terraces, regarding which I shall make the following remarks.

An upthrusting of the land may be conceived to embrace a tract of some extent, either so as to raise the tract equally all over, or so as to raise it in some places more, in others less. It may also be conceived to be quite local, so as to affect only a single place. If a rapid upthrusting of land and a succeeding pause are made the condition of the formation of terraces, it seems that the greater or smaller horizontal extent, the general evenness, and the unevenness of the upthrusting, must have certain consequences as to the level of the terraces above the sea in one valley, compared with their level in the next. If an extensive tract of the coast be raised equally or to the same height all over, it seems likely, if not exactly necessary, that the heights of the terraces at the nether course of one river should, in some measure, correspond to the heights of the terraces at that of the next river, if the rivers do not lie far from each other. If, on the contrary, the elevation be unequal, so as to raise a tract of some extent differently in different places, or if the elevations be local, so as to raise one place at one time and another at some other time, then a correspondence of the terrace-levels in different places is less probable.

The relations between the terrace-levels I shall, with a view to this, illustrate by some instances from Hardanger, where in 1865 I measured the height above the sea of the terraces on the rivers in Odda, Tyssedal, Kinsarvik, Øyfjord and Graven. These terraces, situated partly at the mouths of the rivers into the Hardangerfjord, partly at the river-course just above the mouths, may safely be looked upon as deposited in the sea. The altitudes are as follows:[1])

No. of the terraces from above	The altitude above the sea of the terraces in				
	Odda	Tyssedal	Kinsarvik	Øyfjord	Graven
1st terrace	293 feet.	253 feet.	239 feet.	331 feet.	155 feet.
2nd —	94 —	115 —	205 —	63 —	98 —
3d —	80 —		138 —		47 —
4th —	43 —		71 —		29 —
5th —	12 —		18 —		

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[1]) The numbers giving the mean heights of the terraces are found by careful levelling, accordingly more to be relied upon than if found through the barometer.

However, at such measurements errors are easily incurred, either in the choice of a starting-point on the beach, or in the choice of an end-point on the surface of the terrace. The same errors may be incurred, if the levelling be finished at the lowest or highest point of the terrace. For the limits of the terraces are seldom so sharp, that these points are to be made out precisely. It is possible that some other observer might have found more terraces on these rivers. I only measured the heights of such platforms, whose surface was so large, so even, and so near horizontal, that I thought I might safely call them terraces.

If a right line be drawn from the nether end of the lake of Odda to that of the lake of Øyfjord, from thence one to that of the lake of Graven, thence to the outlet of the river into the fjord of Graven, and thence to the starting-point in Odda, we shall have an irregular quadrangle, within the outlines of which all these terraces lie, and whose area will scarcely attain 4 Norwegian square miles. On this area 18 terraces occur, amongst which not 2 are found corresponding in height. The ground has not then the look of having been raised equally all over. This may, however, have been the case, in the following manner. When the terrace No. 1 i Øyfjord, the highest of them all, lay ready-made in the level of the sea, the land was thrust aloft, then lay quiet. During the pause the terrace No. 1 in Odda, the second-highest, grew up to the surface of the sea. Again the land was raised, then again it paused. During this pause the terrace No. 1 in Tyssedal grew up to the sea-level &c. &c. In this way we shall have 18 upward pushes and 18 pauses, the present one included. How many pushes and pauses are we then to reckon for the whole of Scandinavia, with all its terraces, measured and unmeasured?

The great number of upheavals seems reducible, if one shall imagine the ground unequally raised in the following way: all the terraces No. 1 lay ready-made in the level of the sea, the land was thrust aloft, and the terraces came to lie in an undulating line. During the succeeding pause the terraces No. 2 grew up to the sea-level, then were raised so as to come to lie in an undulating line, &c. In this way one might manage it with 5 pushes. Here, however, is to be observed: the terraces on the same river lie so near each other, that it cannot easily be conceived, how some inferior terrace should have been lifted up, without each of the superior ones in the same locality being lifted as much. From this follows, that the undulating line of the terraces No. 1 is a result of 5 upheavals; likewise, that the undulating line of the terraces No. 2 is a result of 4 upheavals; further that the undulating line of the terraces No. 3 is a result of 3 upheavals, &c.

But first, it must be observed, that each undulating line must have changed its form at each subsequent upheaval. Thus, the undulating line No. 1 or, what comes to the same, the terraces No. 1, must in the different places after the different upward thrusts have lain in the following heights above the sea:

	The height of the terraces No. 1 in				
	Odda	Tyssedal	Kinsarvik	Øyfjord	Graven
After the 1st thrust	199 feet.	138 feet.	34 feet.	268 feet.	57 feet.
— - 2nd —	213 —	? —	101 —	? —	108 —
— - 3d —	250 —	? —	168 —	? —	126 —
— - 4th —	281 —	? —	221 —	? —	? —
— - 5th —	293 —	253 —	239 —	331 —	155 —

Similar changes the undulating lines Nos 2, 3, & c must have undergone; all of which is to say, that the relief of the land has undergone considerable changes at each upheaval, which again presupposes that the part of the terrestrial crust concerned must have been very flexible. Then it must be observed, that either only two upheavals must have occurred in Tyssedal, as well as in Øyfjord, which is against the presupposition, or no terraces must have been deposited in those places during several periods of pause, or the terraces formed must again have been washed away.

That any hypothesis can be found, which, retaining upheavals that raise the whole area differently in different places, explains the relative levels of the said terraces in an inartificial manner, I am inclined to doubt. The simplest hypothesis would be, that the upheavals in one valley took place without affecting the next. But then we should arrive at 5 local thrusts in Odda, two ditto in Tyssedal, 5 ditto in Kinsarvik, 2 ditto in Øyfjord and 4 ditto in Graven. And if the earth's crust should be able to start up 5 times in Odda, without Tyssedal, at a distance of only ½ Norwegian mile, being affected, it must be flexible and movable indeed.

To my knowledge nobody has pointed out a series of terraces in flat tracts consisting of detritus and situated along the open sea or on the fjords *far* from our rivers or rivers' mouths. There can scarcely be any doubt, but a good deal of detritus lies under water in tracts where the rocky ground of the land shoots out in flats under the fjords or the sea, even if the locality should lie far from the nearest river's mouth. Nor can there be any doubt, but in such places terrace-like platforms would come to lie, one above the other, in case the land in the future should rise through quick starts with intervening pauses. Then

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it may be asked, why it should not be possible to find similar platforms from past times on suitable ground along the sea or the fjords far from rivers and rivers' mouths. To this may be answered, certainly, that the ancient upheavals took place only at the nether course of the rivers. But that would be an arbitrary assumption.

To this may be added: As the earth's crust cannot easily be conceived to have been, even so far back as in the Glacial Period, so thin and flexible, that Scandinavia swelled because it was rid of the heavy load of the inland-ice, the repeated starts must be brought into connexion with earthquakes, or laid to the charge of the subterranean forces on the whole. Changes of level connected with earthquakes are known, certainly, especially from volcanic regions. But it has not been proved that volcanoes have existed in Scandinavia since the Glacial Period. On the whole it seems that, if the subterranean forces, little known as they are, are made to be astir thrusting upwards, whenever the surface of a terrace is on a level with the sea, and are made to rest till the next shall have got into the same situation, a rather frequent and free use is made of them.

The theory of terraces treated of builds terraces by means of two factors, the rise of land and the supply of detritus to the sea by the rivers, assuming the rising to be very changeable or intermittent, and leaving it undecided whether the other factor, the supply of detritus, is to be constant or changing. I will now try, whether terraces cannot be built, with the aid of the same factors, by making the former, the rise of land, constant or nearly so, the latter, the supply of detritus, changeable, and by adding a third factor, namely the removing power of the sea.

If, the relative level of sea and land remaining the same, a river carries down more stones, gravel, sand, and mud to the sea, than the latter is able to remove, it is a well known fact that the sea at the river's mouth will silt up, and a piece of flat land with an even surface will grow forth. If such a piece of land exists, and the sea removes more detritus than the river supplies, it is as well known, that the sea will attack this piece of land at the river's mouth and advance at its expense. If the land is slowly rising, while a river during a period of some length carries down more detritus than the sea is able to clear away, the land will not only grow forth at the river's mouth, and that quicker than in the former case, but it will also, during its growth, rise above the sea-level.

If then a period follows, in which the land is in a constant slow rising, while the sea removes more detritus than the river carries down, the piece of land, grown out in the former period and somewhat raised so as to be gently sloping towards the sea, will go on rising, the sea in the meantime attacking its outer margin and giving this a descent more or less steep. During both periods the river is busy cutting its channel deeper into the newformed piece of land. Thus a terrace No. 1 is obtained. A subsequent period having a surplus of detritus carried down, then an other having a minus, will supply the terrace No. 2. If a period arrives, in which the river's supply of detritus and the sea's removal of the same are nearly equal, an inclined plane will originate under the slow rising of the land. The height of a terrace above the next will depend upon the length of the period during which the land advances at the river's mouth, and the length of the period during which it recedes or during which the sea advances, as also upon the surplus of detritus supplied during the former, and upon the surplus of detritus removed during the latter. It is not, of course, necessary to the formation of a terrace, that the river in one period constantly supplies more detritus, and in the next less, than the sea removes; it is sufficient, if the river on the whole has a considerable plus in the former and a considerable minus in the latter.

The aforesaid, in connection with the statements of the American geologist Dana[1]) about the formation of terraces, appears to me to give an explanation of the phenomenon of terraces in Scandinavia. By this theory of terraces one disposes less freely of the subterranean forces, one need not ask whether there were volcanoes in Scandinavia in the Postglacial time, nor be concerned about the corespondence of the terrace-levels of one valley with those of the next, nor has one any obligation to point out terraces in flat and loose ground on the coast far from rivers and rivers' mouths or, such wanting, give the reason why they are not found in localities of this kind. For, if the land rose about equally in the course of time, the question can only be about local causes of the occurrence of terraces in such places — f. ex. horizontal platforms in the rocky ground. On the other side, one is bound to give plausible reasons why a river should carry down to its outlet into the sea, now more, now less, of loose stony matter, than the sea is able to clear away.

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[1]) Manual of Geology p. 555. London 1863.

The quantity of stones, gravel, sand, and mud, which a river bears down to the sea during a certain period, will depend upon a complexity of factors. It depends: 1) on the total of atmospheric precipitate in the period. The greater this amount is, the larger, the more rapid, and the more violent the rivers will be, and the more the detritus; the less the amount, the smaller, the slower, and the weaker will the rivers be, and the less the detritus. 2) On the distribution of this amount over the period. The same quantity of precipitate, supplied during a short time, will give more detritus, than when distributed evenly over a greater length of time. Heavy falls of snow f. ex. will in a mountainous country occasion snowslips, which generally carry great quantities of stones, gravel, sand, and mud into the river; heavy autumnal rains may cause mountainslips, earthslips, a single of which may supply the river with material for building a large terrace, while the same quantity of rain, equally distributed over autumn and summer, would run down to the sea without conveying stony material of any consideration. 3) On the relations of temperature. Intense heat in the first part of the summer will melt the snow of last winter in a trice and may call forth floods, that supply more detritus, than the snow of many winters under a slow thaw. 4) On the nature of the river-basin. A basin containing lakes yields — ceteris paribus — less detritus at the river's outlet into the sea, than a basin without lakes; in a woody basin, floods and inundations do not arise so suddenly, as in one devoid of wood; the former then yields less detritus than the latter. 5) On the nature of the river's bed. A river-bed in loose rocks yields more detritus than one in more solid formations.

It is well known that the athmospherical precipitates vary from year to year, both in the total and in the distribution of it. It is also known that in a particular region, a river's basin f. ex., they may in both respects vary from one series of years to another. It is not known, whether the same be the case with them for periods comprising centuries. But there is nothing unreasonable in imagining this. The same may be said of the relations of temperature.

A lake may be filled up, and the detritus from the superior parts of the river-basin pass on; its dam may burst, and the river may seize upon the store of loose stony material deposited in the lake. A lake, a level reservoir of water, may arise and put a stop to the passage of detritus. In the case of periods of some length, the woods may undergo changes, without the interference of man.

A river — it will in the course of time throw itself from one side of the dale to the other — may in its way come more into contact with loose rocks in one period than in another, and consequently yield more detritus in the former than in the latter. Passing along a water-course, you will often be able to say with full confidence, that in such and such a place lies a piece of ground doomed to be carried off by the stream, and that the latter, having done the work, will for a long distance come to wash a rocky surface which will stand its ground. A circumstance, belonging to the accidental, but of great moment as to the present question, is the fact that a river may, by underwashing its banks, cause more landslips in one period than in the other, thus conveying more loose stony matter in the former than in the latter.

The sea's removal of the loose stony material carried down cannot, indeed, be supposed so variable as the river's supply. Even the sea, however, may, now counteract, now further, the building of terraces, through tempests and calms, ebb and flood, and consequent settings of the current. A syzygial flood, further raised by a tempest at sea which presses the water into the fjords, cotemporary with a calm in the fjord and inundations of the rivers, will greatly favour the formation of terraces. A spring flood, a storm from the sea, and a heavy surf, may in a trice do away with a terrace in building, wash off great part of an older one, and may also throw up the material upon a flat beach, that generally lies out of the reach of the sea. A spring flood and a heavy swell against the shore, may stop up the river's mouth so as to make the river seek another outlet, thus laying the carried-down detritus in other places than formerly. A low ebb and a high surf will make the submarine brink of the terraces in formation advance. In short: a storm from the sea, with a consequent heavy surf, may handle a beach consisting of loose detritus so roughly as to make it difficult to recognize from day to day.

Thus the carrying-down of detritus to the rivers' mouths and the removal of it from them, stand under the influence of an extensive complexity of changeable factors and accidental circumstances. There can be no doubt, the question being of short periods, but the river supplies, now more, now as much, now less detritus, than the sea clears away. And it is no unreasonable supposition, that it may work chiefly in the same way in longer periods. But in this case there will, during the slow elevation of the land, arise a series of terraces with, or without, intervening inclined planes, along the nether courses of the rivers. A period of some length, during which the river supplies more loose stony material than the sea clears away, followed by one during which it supplies less, — then one in which again it supplies more, will leave behind a series of terraces *without* intervening inclined planes. A period of some length in which the river supplies more detritus than the sea removes, followed by one during which it supplies less, — then one in which the river supplies and the sea removes alike much, — then one in which again the river supplies more &c., will leave behind a series of terraces *with* intervening inclined planes.

The formation of terraces, particularly of terraces with great difference of level, may also be occasioned by terrace-like platforms in the rocky ground of the valley. A great difference of level between two consecutive terraces may also arise from the sea having, in the period in which its removal of detritus was greater than the river's supply, washed away the slope formed in the preceding period; it may also arise from the subsequent terrace gliding down from the preceding one. Finally there is the possibility of submarine slips, by which litoral banks subside, leaving a precipice along the strand and forming a terrace or the beginning of one under the surface of the sea, which terrace, when at length emerging, may lie far below that formed immediately before.

As for the length of the time, which the formation of the terraces or, what comes to the same, the land's rising up to 600 feet, may have taken, it is a question for the decision of which I do not think the time come yet, hardly even the time for its preliminary discussion. When Lyell puts the length of the said period at 24,000 years at least, one may say: *Granted!* — *if* the part of Scandinavia in question rose on an average 2½ feet in the last century, and *if* the peninsula rose at the same mean-rate during each of the centuries comprised within the 24,000 years. But there is no surety whatever of the assumptions being correct. And when Professor Kjerulf points to the melting away of the inland-ice as the cause of violent floods and a consequent rapid formation of terraces, the rapid growth of the terraces may well be admitted, *if* such violent floods should have taken place. But then it is not known, whether the glacial covering (or the inland-ice) in the different parts of the earth wasted, because the heat increased, or because the falls of snow decreased; and in either case the disappearance of the inland-ice may be imagined going on slowly, imperceptibly, taking an immense length of time. As long as the Glacial Period stands there an undissolved geological nebula, or as long as the cosmic or telluric causes, which occasioned it and removed it, are unknown, it remains, so to speak, a matter of taste, by what means, how slowly, or how rapidly, the inland-ice shall be made to disappear.

As it is not to be conceived, that terraces can be formed under a mighty gliding mass of ice, it does not touch the question in issue, how it was that the glacial cover shrank, till the point of time when it forked into separate courses and drew back the flaps, it had sent down the valleys, so far as no more to cover the places where the uppermost marine terraces occur. What touches the question is only, whether the shrinking of the inland-ice after the said point of time was slow, or rapid. And on this matter it seems that our vallies above the said terraces must be able to give some information. It seems that the occurrence of many or few, of large or small, moraines, or the absence of moraines, in these vallies, must give hints about the rapid, or the lingering, retreat of the glaciers within their present boundaries, and at the same time about the rapidity, or the slowness, of the melting away. Our vallies, however, are little examined with a view to this. I have examined some rocky vallies in the western mountains. In these vallies the moraines were neither so many, nor so large, as to intimate a protracted retreat. It cannot, however, be looked upon as settled, that the weak development of moraines in those dales is to be accounted for by the rapid melting and the quick retreat of the glaciers. It may also be accounted for, partly by the rocky ground being so firm as to supply the material for building moraines only sparingly, partly by the absence of storing-places for this material.

Seen from other points of view, Scandinavia discovers divers signs which should seem to point out that it cannot be so very long, geologically speaking, since the uppermost marine terraces lay on a level with the sea: as our waterfalls, our many lakes, and the great depth of the western fjords, when compared to that of the German Ocean. Every watercourse works at doing away with its falls, filling up the lakes it lights upon, and silting up the fjords which it enters. If a very long period should have elapsed since the completion of the uppermost marine terraces, our rivers would of necessity in the course of this time have worn down their falls, as well as the Indian rivers, in whose way from the lofty Himalaya and from Thibet, according to Schlagintweit, scarcely any waterfalls are found. During the same period the lakes would have been filled up and the fjords silted. But to this may be observed, that our rocks are generally only with difficulty worn away by water, and that our rivers are nearly inactive during the long winters, for which reason the erosion is slow and gives little detritus towards filling the lakes and the fjords. Besides, it is not known, what masses of loose stony matter may, during the said period, have been deposited on the bottom of the lakes and fjords. These masses may have a thickness, that bears witness to the immense length of the time during which they accumulated.

A „giant's-kettle" (chaudière de géant) with its appurtenances, which I found in 1865 on the north-west point of the island of Varaldsøy in the Hardangerfjord, awakened in me an overwhelming conception, vague as of course it was, of the length of the time, its formation must have taken.

The giant's-kettle lies some few feet above the sea, is about 3 feet deep, and its inner side runs up into a vertical semicylinder, which I from an eye-measure should put at 120 feet high. According to the common notion about the origin of giant's-kettles, this semicylinder, together with the giant's-kettle, must have been formed by an eddy in the level of the sea. Accepting for a moment the estimate of Lyell, that Scandinavia used 24,000 years to rise 600 feet, it must also be accepted, that it used a space of 4920 years to rise the 123 feet, which are the height of the semicylinder plus the depth of the giant's-kettle, during which time the semicylinder and the giant's-kettle must have been formed; this involves, that the erosion, or the excavating work of the eddy, advanced 1 foot in 40 years, and 1 inch in 3⅓ years, on an average. In connexion with this I shall observe, first, that the eddy cannot easily be imagined to have existed and worked continuously in a fjord, where the currents run, now in, now out, with the tide. It would, at all events, be a not very plausible supposition that the eddy not only turned exactly when the current turned, but also bored on into the rock, in whatever direction it went round. Secondly, I shall observe, that the stony matter an eddy works with generally does not scour, but roll, round in the place of work, thus only feebly attacking the rock. Further, it must be observed, that this stone, or this stony matter, could, on account of the situation of the working-place, only with difficulty and at intervals be supplied, when worn out. For it must be presumed to be conveyed to the place from afar, especially from the north. That the stones and the gravel found in the giant's-kettle were conveyed to the place, cannot be doubted. The conveyance cannot have taken place on a glacier. For at the time, when the land lay only some 130 feet lower than at present, the glaciers must long ago have ceased to run out into the fjords. The glaciers must of course have receded from the beach already at the time when the land lay 600 feet lower than now; else marine terraces could not be met with at 600 feet above the modern coast-line. Nor can the conveyance have been effected by the current along the bottom of the fjord. For the sea is deep all around Varaldsøy, except on the south side. On the north side I did not reach the bottom with a line 1800 feet long. Thus, the conveyance of the stones and gravel, with which the eddy excavated the semicylinder and the giant's-kettle, can only be imagined as effected on common fjord-ice, which at present very seldom, if ever, strikes against Varaldsøy. But that the ice should, on the rather broad Hardangerfjord where the current may carry it out past the island both to the west and to the east of it, have often transported stones and gravel to a single point or to a circumscribed locality of the island, is not very likely; so it may be imagined, that the eddy has often been in want of tools for long times. That, under such circumstances, the eddy should in 3½ years have scooped out on an average 1 inch in a rather hard rock, seems to be more than might have been expected. Thus, Lyell's estimate of time seems, in the present case, to be rather too short than too long.

As it is not, however, unquestionable either that the semicylinder and the giant's-kettle have originated in an eddy, or that the latter, if so be the case, occurred in the surface of the sea, what has been stated about them cannot assist very much in defining the notions about the length of the time in question. On the contrary, it seems to me, that what has been said about this semicylinder and this giant's-kettle, taken together with the preceding observations, is of a nature to support the view, that the present stage of the observations lies too low and is too loose for the distance between the Glacial Period and the present one to be measured from it.

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